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THE JOURNAL OF PHILOSOPHY

EMPIRICISM VERSUS FORMALISM IN WORK WITH MENTAL TESTS

THE past four or five years have seen a simply extraordinary activity in the field of "mental tests." "Testing" has, however, developed in a curious fashion largely without definite scientific affiliations. As one result, critical discussion of fundamental principles has lagged far behind application. And work in this field is tied up with large hypotheses which have never received adequate critical consideration. As one of a number of recent efforts toward a critical review of methods, the writer has lately published a paper entitled—somewhat ambitiously—"Suggestions Looking Toward a Fundamental Revision of Current Statistical Procedure, as Applied to Tests."[1] He has just received from Dr. Ruml a proof of a paper commenting on this article.[2] With the proof was a letter in which Dr. Ruml expresses the hope that a reply to these comments may be early forthcoming; he feels that discussion upon the mental principles in test work is at present most desirable. With this last statement, at least, the writer is in full agreement; and he hastens to make such reply. The issues can, he believes, be put very briefly. They are three in number.

1. The fundamental contention of the writer is for a more empirical method, in work with tests, and less easy assumption of large hypotheses. For instance, it should be obvious that if the validity of the theory of general intelligence is assumed, and results on different tests summed to make a total score, if teachers or employers are asked to estimate "general ability," if marks in school or college are averaged to give general ability in school work, little evidence contrary to the theory of general ability can be found. The whole situation has been generalized in the first place. The concept of general intelligence is, as a matter of fact, being very seriously questioned at present.[3] The contribution, to this fundamental

[1] *Psychological Review*, Vol. 27, pp. 466, 472, November, 1920.

[2] Beardsley Ruml, "Reconstruction in Mental Tests," this JOURNAL, Vol. 18, pp. 181–185, March 31, 1921.

[3] For an admirable non-technical account of some recent conclusions in the matter, see E. L. Thorndike, "Measuring Human Intelligence," *Harper's Magazine*, Vol. 140, 1920, pp. 227–235. The same writer summarizes some interesting correlations in a brief paper "Reliability and Significance of Tests of Intelligence," *Jour. of Educational Psychology*, Vol. 11, 1920, pp. 284–87. An interesting discussion from the point of view of the employment psychol-

problem, of the tremendous amount of work with mental tests is really astoundingly small, simply because the work has been so tied up with the hypothesis.

The same situation holds with regard to the use of the theory of the " normal " curve, in test work. It is common practise, in building educational tests, to assume that the ability in question is distributed according to the " normal " curve. The items are, then, selected in the first place to give equal units on a " probable error " scale. And, naturally, the results given with the test show a " normal " distribution. The investigator then goes further (very likely) and works out supposedly equal units of ability, calculates a " zero point " in the ability in question, as measured by the scale, and talks easily about " silent reading ability " or general intelligence, in the same fashion that he might discuss measures of height or weight. As a matter of fact, the concrete facts of the situation are so overlaid by the theory which has been involved in the treatment that the validity of the theory for the particular ability in question can not be determined, nor the application of the theory to the practical problem concerned.[4]

Other large assumptions, perhaps less vital, but no less unfortunate in principle, are common in present statistical practise. The involvement of the test work in such hypotheses is surely unfortunate. It is also decidedly unfortunate that such methods have led to the development of a formal statistical method; and we find effort that should be devoted to the study of fundamental problems being wasted, instead, in the fulfilment of formal statistical requirements. An astounding amount of time is being devoted to the development of " units," " zero points," and " weightings "—to the mystification of the uninitiated, and to the neglect of elementary problems of analysis and verification.

When a situation is thus clouded by hypotheses, a return to a rigidly empirical method is naturally suggested. A mode of attack must be worked out which will be free from such doubtful hypothetical elements. So the writer has argued (to Dr. Ruml's horror, apparently) for, " no hypotheses, as thoroughly empirical treatment as may be!" Surely such a return to rigidly empirical

ogist will be found in Henry C. Link's *Employment Psychology*, Macmillan, 1919, Chapter 12, " General Intelligence, a Dialogue," pp. 130–139. Consideration of theoretical aspects of the matter will be found in articles by G. H. Thompson, " The General Factor Fallacy in Psychology," *British Journal of Psychology*, Vol. 10, 1920, pp. 319–26.

[4] See, for instance, E. G. Boring, " The Logic of the Normal Law of Error in Mental Measurements," *American Journal of Psychology*, Volume 31, 1920, pp. 1–33. The paper is, the writer believes, of the very greatest theoretical importance.

methods would seem the natural and healthy way out of such a situation. It would seem the only way in which present hypotheses might be verified, and new and more sound hypotheses developed.[6]

2. The writer's second point has to do simply with a matter of fact. *As a matter of actual fact,* " testing " is at present largely a technical science. The writer has argued, as strongly as any one, for purer research in the field of mental tests and for the need for such research.[7] He is now devoting more time than he should, perhaps, to such work. However, *as a matter of actual fact,* work with tests is now largely of an immediately practical nature—or at least, is with reference to practical problems. Under such circumstances, certain very definite and very practical obligations are incurred. Suppose one is using tests of intelligence in order to determine whether a boy in the fourth grade may not be capable of doing fifth grade work; the hope is, that by a little special coaching and " double-promotion " the child may ultimately be saved a year in his school work. It is no abstract child who is under discussion—it might be your child. The one consideration which is paramount to all others, under such circumstances, is the very practical consideration—which tests, and which methods, will aid most effectively in deciding this practical question. Dr. Ruml comments, in horrified tones, on the fact that the writer " would even use his statistics to produce bimodal distributions," in order to deal most efficiently with such a practical problem. Certainly the writer would be quite willing to sacrifice formal statistical methods in order to deal more effectively with the boy.

The contrary practise is, unfortunately, the common practise at present. The one great question would seem to be: just what will these tests tell with regard to most satisfactory grade placement? How well (the question is) will the test inform us as to whether this boy can do the work of the fifth grade, and what is the significance of a score of 20, in this matter, as compared with a score of 15? Instead, investigators are putting their best efforts into finding out whether a score of 20 is as far above 15 as 15 is above 10, and how far these various scores are above " zero " ability.

[6] Ruml issues a solemn warning against any such " direct action." The empirical method as "direct action"! Such strictures have, the writer believes, been put upon efforts at a return to first facts, before.

[7] See especially S. L. and L. W. Pressey, " Cross-out Tests, with Suggestions as to a Group Scale of the Emotions," *Journal of Applied Psychology,* Vol. III, 1919, pp. 138–150. Also, S. L. Pressey and O. R. Chambers, " First Revision of a Group Scale Designed for Investigating the Emotions with Tentative Norms," *Journal of Applied Psychology,* Vol. IV, 1920, pp. 97–104; and L. W. and S. L. Pressey, "A Critical Study of the Concept of Silent Reading Ability," *Journal of Educational Psychology,* Vol. XII, January, 1921, pp. 25–31.

It is such substitution of formal statistical requirements for practical requirements, in use of tests for dealing with practical problems, to which the writer particularly objects. Practical efficiency is certainly the criterion by which our statistical technique should be judged, if we are dealing with practical problems. Our obligation is not to the requirements of a formal method, but to the needs of the people with whom we are working. If a "statistical trick" will inform us more accurately as to whether the boy above mentioned belongs in the fifth grade, then by all means use the "trick." Instead, it more often happens that the best welfare of the boy is sacrificed to the requirements of statistical pedantry.

To sum up then: the writer wishes (1) a return to strictly empirical methods in view of current injudicious and uncritical use of hypotheses, and (2) a recognition of the practical obligations involved in use of tests for practical purposes, and the development of statistical methods designed specifically with reference to these practical problems. Now, a word with regard to the type of method which, to the writer's view, should give the empirical approach desired, and as to the relation of such a method to testing as a technical science. It must be emphasized that any faults of this method do not necessarily comprise the truth of the first two contentions; the writer insists that more empirical method and a more consciously technical treatment for technical problems, are desirable; any faults of his particular type of empiricism, or of his notions with regard to the relations of empiricism to a psycho-technique, must not be allowed to compromise the main issues. However, the first two contentions may be made clearer if some positive suggestions as to method are added.

3. It is first necessary, as the writer sees it, that methods be freed from involvement in hypotheses as yet not thoroughly verified (a requirement that certainly leaves very few hypotheses which may safely be thus employed!) To return to the previous example, it should not be assumed that the same tests may be used in prognosticating success or failure in learning to read, and in learning arithmetic. Each problem must be dealt with separately. If it appears that the same tests may be used to indicate capacity along these two lines, well and good. But this must not be assumed. There must be specific study of specific problems, and always an opportunity for analysis.

There must also (in such empirical procedure) be a willingness to work with materials as they are, without transformation into terms more readily treated according to current statistical practises. To return again to a previous example, it may be that "arithmetical ability" is distributed according to the "normal"

curve. It may well be that problem-solving is at present taught in the school in such a haphazard way that the operation of chance factors does bring about a " chance " distribution. But one must not proceed to do all his thinking with regard to the teaching of arithmetic in these terms, from now on. The aim, in the teaching of problem-solving, is to equip the child to solve common practical problems in later life; and there is no evidence that such practical requirements are so distributed. In fact, the writer suspects, it is a fairly reasonable inference that the most efficient school would bring about a distribution which would not be " normal." Moreover, schools are organized at present primarily with reference to minimal requirements. And the fundamental unit is the grade. An empirical treatment will seek to construe the data with reference to these various facts, rather than in terms of a biological or statistical theory.

And here we come (if the writer understands Ruml correctly) to the third point of difference between Dr. Ruml and himself. Such empirical methods have the great advantage of also getting closer to practical problems. In other words, the writer believes that the study of practical problems will not lead one to " ignore the necessity for analysis," but will bring about analysis; will not " warp the observed data," but will describe the data as they are. Such methods will " warp " the results into a shape not congruous with some current theories, to be sure. But that is not proof positive that these methods are wrong. One can not but wonder whether Dr. Ruml's difficulty may not be that he is thinking entirely in terms of " Pearsonian statistics " while we need a " relativistic " theory.... Anyhow, the writer refuses to be frightened away from the greatest possible efficiency in his technical work, because of the charge that these most efficient methods are unscientific. Rather, he believes that the efficiency of a method is not such bad evidence of its fundamental soundness. And he will be inclined to take his cue from this, and aim first at the greatest usefulness, in test methods—with the confidence that such a criterion could not lead one very far astray.

Well—so much for the general concept, as the writer sees it. However, it is obvious that his theory, essentially, can not rest with a general statement but calls for a pragmatic proof. The question is as to what, actually, such an approach will yield. He will have in print, shortly, test materials developed and handled precisely after the method suggested in the previous paper.[8] It is

[8] In fact, he has already presented something of this sort in previous papers. See, for instance, S. L. and L. W. Pressey, " Irregularity in a Psychological

about such concrete matter rather than about general statements of theory that discussion would seem most profitably centered. That there should be such discussion seems to the writer most highly desirable; he is in thorough agreement with Dr. Ruml on this point. In fact, he hopes, with Dr. Ruml, that others may take part in the discussion. He also hopes, with him, that any such discussion may proceed without any of the personal irritation which sometimes so unhappily develops, in the course of differences regarding matters of scientific method. There is surely a need for the clarification of both aims and methods in the field of tests. Such clarification is most decidedly needed, if test work is to continue its healthy growth. If "testing" can thus be brought into its own, the writer believes, as stated in a previous article[9] that the test method will be of great value as a method in pure research. Meanwhile, testing *is* primarily, at present, of a practical nature; and the writer feels that the most healthy and consecutive development, from both the scientific and practical point of view, can most economically be obtained by a vigorous, persistent, and open-minded carrying through of the practical problems to which we are now obligated.

S. L. PRESSEY.

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A NOTE ON THE RÔLE OF MATHEMATICS IN PHYSICS

I

BECAUSE *static* phenomena furnished the model upon which Cartesian geometry was constructed, Leibniz and Newton, especially the latter, were compelled to develop or use a mathematical theory built upon the model of *dynamic* facts as supplied primarily by Da Vinci, Copernicus, Galileo and Kepler. If it is fertile, calculus, as well as any other system of mathematical theory, should bring to light new facts about the physical phenomena as a result of which it was originally constructed. Furthermore the new facts give rise to a new model of physical phenomena. In fact, every new physical fact gives birth to a new model of physical phenomena, upon which mathematicians develop new mathematical theories, which in turn give rise to new generalizations, and if taken up by physicists are the means of discovering new existential facts.

Examination as a Measure of Mental Deterioration," *Journal of Abnormal Psychology*, Vol. XIII, No. 5, December, 1918, pp. 285–294; and S. L. and L. W. Pressey, "The Practical 'Efficiency' of a Group Scale of Intelligence," *Journal of Applied Psychology*, Vol. III, March, 1919, pp. 68–80.

[9] See note 7.

That is, if we become acquainted with a variety of simple experimental facts, we at once begin to connect these facts, if at all possible, into a coherent system; that system becomes an "*ideal*" model in which each and every fact is in some way related to each and every other fact within the model. A new model will lead to a development of a new system of mathematical theory, which, if perfect, may be reduced to a small number of constants and simple equations, and if the latter are developed further, they will lead to new theorems which again may produce new discoveries of physical facts. Therefore it must be said that in their development mathematics and physics depend upon each other to a very large degree. It is physics which may supply to mathematics new models and therefore be indirectly responsible for the development of new mathematical theorems or systems. Mathematics again, because of its generalizations, does not only extend to its own field but also supplies to physics ideas which may directly lead to new discoveries. For example, Newton always began with an analysis of phenomena and then used mathematical synthesis in order to discover new physical facts.

Mathematics plays always an instrumental function in physics, but its rôle is more than merely that of an instrument; it also makes known the *class* to which certain facts belong, but never their *essence*. To believe with Descartes that with matter and motion one could mathematically construct a model of our universe is only an *a priori* idea and not an empirical fact. Even though Cartesians claimed that Descartes's geometry rested upon metaphysical axioms and that the development of these axioms would answer each and every question that physics may raise, some questions could not be answered satisfactorily, consequently a new mathematical system was developed. That does not mean that this new system has taken the place of the old system of theorems, but rather that this new system has taken a place beside the old one, and as a result of this mathematical science was enriched. Descartes defended the mathematical theory of *continuum*, which he based upon a physical model of continuous matter. It was necessary to develop a new mathematical theory when matter was discovered to be discontinuous. The doctrine of molecular discontinuity gave rise to a new model which was instrumental in the development of new mathematical theorems. Once these theorems are deduced, they become entirely independent of the physical theory of molecular discontinuity.

It was Newton and not Descartes who pointed out the function that mathematics must play in discovery. Mathematics must not rest upon axioms which are derived *a priori;* they must be a result of experience, they must be generalizations from models of nature

furnished to us by our experience of facts, whether it be in the street or laboratory. It was Newton who showed by example that we must first analyze facts of experience in order to construct our model, which can be synthesized when mathematically developed. The axioms or definitions are results of analysis (mathematical analysis) which when connected systematically (mathematical synthesis) give rise to new generalizations (due to mathematical deduction) in turn giving rise to new discoveries of physical facts. The truth of these generalizations is directly dependent upon our constant guidance by experience in our procedure.

It would be fallacious to think however that mathematics adapts itself to physics, any more than algebra adapts itself to geometry and gives rise to analytical geometry. The fact is that mathematical physics is the true physics, a fact long recognized by Newton. Mathematical physics is not in any sense opposed to experimental physics; the former is the "*ideal*," towards which all development of experimental physics should tend. All experiments are means which lead to mathematical explanation; that is actually the true function of mathematics in physics. It is the experiment which gives value or truth to experimental physics, but in its perfect and most useful form it is expressed in mathematical language and consequently the fact of experimental physics becomes the fact of mathematical physics.

Therefore the rôle of mathematics in physics is not that of a discipline independent of facts, and mathematics does *not* give us truth *a priori*. All mathematical physics must begin with facts of experience. To explain fully what may be meant by truth in this connection would necessitate a discussion of the relation of physics to metaphysics, which will be the task of another paper.[1] The exact rôle of mathematics is not to establish mathematical science or mathematical theorems; no more is it to establish *a priori* connections between different facts. These could be known empirically. The rôle of mathematics is to make the connections more easily obtainable and to serve to discover and to express laws, not to prove them, and above all to prove that they have an eternal value.

II

It was Newton's conception of particles or atoms as mathematical points that became so fundamental to all future mechanics, even

[1] We may point out briefly in this connection that it is due to Newton's doctrine of symmetry and not to the Leibnizian principle of sufficient reason that prediction in science is possible, and that truth in physics is intelligible. It is because of the symmetry in the objects of experience themselves that we can rightfully expect a symmetry of effect from a given cause, and not because of the existence of a rational order of principles, which guarantees deduction.

though he thought of them only as mathematical and not as metaphysical entities. That is, Newton, like Galileo before him, thought that the fundamental properties of matter are those only which lend themselves to quantitative treatment. As has been pointed out above, mathematical theory is the result of models constructed from physical facts of experience, and if this mathematical theory is developed, it gives rise to generalizations which give rise to new ideas, when mastered by physicists. These ideas lead to new discoveries and finally change or develop the original model. Consequently, it is natural for the physicist to develop a mathematical theory based upon the quantitative properties of matter, which alone lend themselves to mathematical treatment. These defined properties are derived from experience. A further development of this mathematical theory should lead to new general concepts which in turn give rise to additional or new properties of matter, *e.g.*, system of forces, centripetal force, etc.

Therefore when forces are conceived as applied to each particle of solid, they may also be conceived as applied to a point of the solid called its center of gravity. The summation of these forces in a single point is called by Newton a centripetal force, to which he gave a mathematical expression actually a natural development of Kepler's laws. Newton defines force in terms of algebra, as an expression of an element in movement measured in an element of time. Force and space are in functional relationship, which is scientifically expressed in mathematical terms. Movement is associated with force, and force is a mathematical concept. It was Galileo who gave to Newton the idea of force, and the idea that gravity is a field of such forces, upon which model Newton constructed his mathematical equation. It is very natural for a mathematical physicist to conceive of a body as made up of physical particles between which certain internal actions or forces are constantly working; so that a body is reduced to a system of points and forces. By doing this, the problem of motion and equilibrium resolves itself into an application of the principles of mechanics of the particle. Or the forces of a body, in fact, resolve themselves into a summation of forces, as the force of an immovable center of gravity of a body. Within any such mass, particles or atoms have a geometrical characteristic which is constant, no matter what forces we may apply to them. Consequently it is very natural and fruitful to assume certain rigid relations between particles of any mass, which are regarded as a system of mathematical or physical static relations, and are adaptable to a mathematical treatment, which actually is an *"ideal" simplification* of physical phenomena. But since natural science, *e.g.*, physics or astronomy, deals with large

aggregates of particles, the heterogeneity of relations which *may* actually exist between these particles is negligible, and a single simple mathematical expression of these relations is not only possible but also very desirable because of its utility. As for example when we assert that an ellipsoid with a great number of dimensions can be defined sufficiently by five or six constants, we mean only that our study of mathematical theory, modeled after ellipsoids, can be expressed by means of these five or six constants. Mathematical theory therefore can be reduced to a small number of equations which involve a small number of constants, but it would be folly to say that the actual physical models are so simple in structure. We may notice another example of *"ideal simplification."* When we express the rate of emission of the α particle, which as a matter of fact it has been actually impossible to see with accuracy, what we really express is the *"mean number,"* and that is really the only thing that we have scientifically arrived at. Here physical facts are only "mean numbers" and these are only the facts accessible to our observation.

We say that A is a function of B if A changes with B, and we can calculate the derivative which will represent this rate of change. We say that this derivative represents the law or rate of change of the body, or in other words, that this derivative stands for the relationship which exists between A and B. But if we carefully observe the change of A and B, whether it be in a physical, chemical, biological or psychological laboratory, and plot the curve representing this functional change, we find that the relationship between A and B is not at all as absolute as the principal concept of calculus has it; in fact the change plotted does not at all represent a curve. What we really have between our X and Y axes, as a fact of actual experience, is a number of points widely distributed, but yet within certain limits representing a continuously progressing curve.[2]

Because the bodies and their motions studied by physicists are apparently so nearly homogeneous, it is therefore possible for them to employ mathematical generalizations of relations in order to express their behavior and nature. It was necessary for Newton to assume that particles are homogeneous, and the models of nature based upon the models of experience dealt with lent themselves to that end. This was necessary, because if the particle (or atoms) were supposed to be heterogeneous no mathematical or any other simple treatment of them would be possible. And furthermore, it was not only necessary to admit the above generalizations, but also that the resultant action of a body would be the direct sum of

[2] I have more fully discussed the above in the *Monist*, for October, 1919.

the actions of all its parts, and that all bodies being made up of similar homogeneous atoms would behave alike. For a generalization contrary to the one stated above would permit any single and simple equation summarizing the behavior of matter, modeled after a large number of diverse facts. That is the reason why the statistical method as employed in science is so fruitful, it always supplies to mathematics the indispensable models, which are constructed on the basis of observable facts which really are the "mean numbers."

Again it is mathematical theory which can bring together under a single generalization the crude facts of isolated experience so different in type as, *e.g.*, the motion of solid bodies, of light, of sound, or of matter.

If the main business of physics (or natural science in general) is to discover and describe the order characteristic of its subject matter, to describe the past and to predict the future with a certain amount of accuracy, then it is legitimate for physics (or science in general) to assume that bodies are functions of their minute units. and that the behavior of a body is the resultant of or a function of the behavior of its units, or that a number of isolated facts can be expressed by a single equation even though this mathematical equation be based upon a model arrived at by a "mean number" statistically derived from observed facts of nature.

A. J. Snow.

University of Pittsburgh.

REVIEWS AND ABSTRACTS OF LITERATURE

Implication and Linear Inference. Bernard Bosanquet. London: Macmillan and Co. 1920. Pp. ix + 180.

It is fabled of the hoop-snake that once upon a time he grew discontented with that linear mode of progression so long affected by his ancestors. Thereupon he took the tip of his tail in his mouth, thus producing in his form that endlessness which is the essence of the good infinite and proceeded to roll like a hoop. So alarming was the nimbleness to which he now attained, that he threatened to become the terror of man and beast. Happily for the rest of creation, he chose to attempt the ultimate mountain heights, where the roads are narrow and tortuous. And there it came about, that the very impetuousness of his velocity caused him to skid on an awkward curve, and he tumbled afar into the abyss.

We have before us a volume of hoop-snake logic. Our author is critical of the linear "bead-theory" of deduction, which has been the dominant one in the development of deductive logic, and which

hangs a random bunch of axioms or postulates in mid-air, without any further visible means of support, and depends therefrom a loose-waving string of deduced theorems. Our author would have every train of deduction be like the lines in a well-composed painting, never losing itself over the borders of the frame, but rather leading the eye always sweeping back to the centers of interest. He would leave no loose ends. He would, on the one hand, assume no axiom as evident in itself alone. Yet neither, on the other hand, would he leave anything finally hypothetical. The deductions would support the principles, as the principles the deductions. Each partial system would, as a whole, fall into its place in a larger system, and this into a larger system still, until all were bound together in that widest, yet unified, circle and synthesis which is the categorical system and totality of all truth—wherein the hoop-snake, having finally swallowed himself, would be transformed into a beatific vision.

To remind ourselves that this point of view is not altogether novel, let us recall a well-known passage from Carlyle: "Our Professor's method is not, in any case, that of the common-school Logic, where the truths all stand in a row, each holding by the skirts of the other; but at best that of practical Reason, proceeding by large Intuition over whole systematic groups and kingdoms; whereby, we might say, a noble complexity, almost like that of Nature, reigns in his Philosophy, or spiritual Picture of Nature: a mighty maze, yet, as faith whispers, not without a plan."

Obviously such a book as that of Mr. Bosanquet's, though readably enough written, will appeal less to the beginner in logic than to the special student, whether in logic or metaphysics, whose previous thought, and previous acquaintance with the wide range of authors criticized, will give him some check on, and some estimate concerning, the force of the argument here presented. In many of these matters a hair-line is all that separates the true and the false. The exact nuance is everything. Yet a reviewer is expected to speak as one having authority, saying, " Here and here all competent students will admit that our author is wrong." I choose rather to write in the first person, as one diffident and tentative, saying only, "Thus at the moment it appears to me."

Realism and idealism, in the philosophical as opposed to the literary sense, differ, I suppose, with primary regard to the place or centrality they respectively give to mind in the world. Only a few years since we were hearing much from realists called new, who would bring the mind more abroad among things than the older, but by no means obsolete, dualistic realism. Perhaps we might venture to say that new realists and idealists are alike in this, that

they are both mentally out of their heads, though what in the new realist is but an eccentricity of the ego, becomes with the idealist a megalomania. But when you come down to arguing the pros and cons of this sort of question, it has a way of transforming itself into prior questions. These latter turn out to be questions rather of principles of logic, wherein nothing is apparently even hinted at concerning the nature of mind. The idealists are pretty uniformly characterized by this, that they place the whole always prior to the part. A consistent idealism seems necessarily to turn into absolutism. The realist is led to say, on the contrary, that the whole presupposes its parts, and there is self-contradiction in defining the parts in terms of that whole which contains them, for the latter can not exist till the parts of it are one and all present. I admit there are those who dismiss the question with a remark that the whole-part relation is not objective. But they are either endorsing the idealist position, or affirming a belief in nominalism—which latter belongs pretty largely to another order of considerations altogether. So our dichotomy remains exhaustive.

Among English writers, the supreme work of idealistic logic is still Mr. F. H. Bradley's *Principles of Logic.* Bradley is too modest in his ascription of superiority to Bosanquet's subsequent logical writings, as Bosanquet would probably be among the first to admit. Bradley's own later book, *Appearance and Reality,* has been more widely read, and the ironical skepticism by which Bradley has there accomplished what some have called "the disappearance of reality" has perhaps done more to discredit metaphysical idealism in the present generation than was effected by any hostile critic of idealism whomsoever. I can not help thinking that the Bradley of *Appearance and Reality* has seen more clearly whither those fascinating and plausible doctrines of the *Principles of Logic* were leading than has the less skeptical Bosanquet. Be this as it may, few who have carefully studied Bradley in his logical masterwork seem to have ever been able again to shake off the grip of his influence. Overeager is Bradley at times to bolster up some foregone thesis, but he is genuinely empirical too, and willing to show on occasion a scupulous respect for fact, such as I have not always observed in those system-scouting gentlemen, who, dismissing Bradley as of course a tissue of *a priori* constructions, thereupon regularly preface each new outpouring of their own *a priori* ideas by the reassurance that they are experimentalists and pragmatists, unwashed empiricists, racy of the soil of reality.

Bradley's theory of judgment, as is well known, makes every judgment the affirming that a certain predicate applies to a subject, emphatically insisting, however, that the subject in question has

ceased to be the usual grammatical subject, and has become the common subject of all judgments, namely, Reality itself. If you say, "Smith hates Jones," you really assert, "Reality is 'Smith hating Jones.'" But, of course, this is not quite true, for Reality is this and a great deal more besides; and you must hasten to complete this judgment by telling more fully what Reality is, lest you be caught speaking half-truths. And then this judgment begins to fuse with other judgments, which have also started along the same road towards completing themselves, and you find no place for pause short of a synthesis of all judgments into one grand predicate, the explication of the absolute totality. Since there is only one Reality, there can be truly only one judgment and one act of knowledge.

It would be a long task to criticize this theory in all its bearings. I restrict myself to a mere suggestion. Personally I should like always to use "real" as a predicate, and never as a subject. The nearest to the Bradley inversion that I would care to make, would be to turn "Smith hates Jones," not into "Reality is such that Smith hates Jones," but into "There is a complex which is real, such that 'Smith hates Jones' describes in part this complex." Whether or no metaphyical monism be true, the question before us concerns what we really do mean when we judge. It seems to me unmistakable that at the core of our actual judgments there is always a specific and limited objective reference. We are intending to characterize, not all Reality, but some specific bit that is real. But is not "all Reality" there, in the background, like the chorus in an opera? So Mr. Bosanquet thinks. We may be so sure, for instance, of an implication or other bit of knowledge, that we venture to say, "If that is not true knowledge, nothing else is," just as we might also say, "If that is not so, I'll eat my hat." Mr. Bosanquet ignores the metaphysical implications of the latter statement, and concentrates on the former, which he neatly transforms into, "That is to say, an implication rightly judged is guaranteed by the whole system of reality." We may all admit that where there is a system, there is the possiblity of guarantees by deduction. But whether "all Reality" is a system tight enough to guarantee anything specific, seems to me most dubious. And in any case, the specific reference is one thing, the guarantee another.

But this is, then, to fall back into the exploded correspondence theory of truth? I shall admit it, yes. I admit it because I am one who believes there are numerous different acts of knowledge, and not one supreme act of knowledge that somehow exists in various mutilated editions. I hold that two minds may exist which know the same thing to the same extent and yet are not in so far forth identical. To believe this is to be a realist; to deny it is to be an

idealist like Mr. Bosanquet. I think it can be proved that other sorts of idealists are inconsistent. As a realist, I am crude, and superficial, and naïve enough to believe, that if I now judge, let us say, that "Confucius was a wise man," what makes my judgment true is not its coherence with other judgments of mine, but is some sort of correspondence with the actual career of a certain benevolent gentleman who lived on the other side of the earth these many years ago. It seems to me just plain nonsense to call this knowledge of mine timeless, or to call it not mine—and I had rather be naïve than nonsensical. I am well aware that mighty arguments have been marshalled against such a crass dualism, arguments that have ranged from lofty assertion down to withering contempt. I admit that all proof I can ever hope to get for the truth of my judgment about Confucius must consist in its coherence with other now present, or some day to be present, contents of my knowledge, and this coherence is my working criterion and guarantee of such truth. Why, then, not drop this irrational yearning after correspondence with something unreachable, and retain only the internal coherence? To this I reply that Confucius is not unreachable. It is through my knowledge that I reach him. He is what my knowledge is about. But just because I reach him only through this knowledge, and in so far as it reaches him and is adequate to him, and I have otherwise no hold upon him, therefore I can not set him on one side, and knowledge on the other, and compare. In this sense the test of knowledge must lie within knowledge itself. But what is there wonderful in this?

My knowledge must not, however, even at its best, be identified with the existence of the object known. Not only does this follow from the proposition laid down above, that there can be two distinct knowledges of the same thing, but there are other reasons perhaps more obvious. Knowledge can not be so much creative of the thing itself but that error is possible and ignorance is possible. The erroneousness of error simply can not be merely partialness. I am willing to grant to Mr. Bosanquet that every false judgment contains some inkling of the truth. But the error part of it is a specific wrong combination. The "right combination in the wrong place" is just a plain wrong combination. But more difficult still is it for a strict coherence theory to explain how ignorance is possible. On a strict coherence theory we ought to be ignorant of nothing, for that of which we are ignorant is surely dualistically apart from us, and by this theory such can not be. The idealist in Berkeley's dialogue replies to the suggestion of a tree in the desert of which no one is thinking, by remarking that his interlocutor had to think of it even in adducing the example. Doubtless, if I were an idealist,

I should be convinced by this clever fallacy. And because I should try to be a consistent idealist, I should go on to prove, in the same manner, to any doubting Hylas, that he was not so ignorant as he had modestly supposed. I should say to him, with a malicious gleam in my eye, "Tell me what you are ignorant of." I should then trumphantly point out, that since he had told me just what it was, he was obviously not ignorant of it. But if he rashly told me he did not know what he was ignorant of, then, as a good idealist, I should indeed have him at a disadvantage. Was he not himself admitting he did not know what he was talking about? — Alas! the facts break in upon our dialectic. We are ignorant of many things. When we learn of them our knowledge must therefore be something superadded. It may make a difference. But then the difference made is a superadded difference. Ignorance and error are facts. I regret they are. But I can not accept a theory that leaves them out of account, and that is what the coherence theory does.

Though I hold that the idealist coherence theory of truth does not correspond to our most ordinary ideas of truth, I should also wish to insist that it does correspond to certain common and useful notions about the truth. If we are aiming at truth, and truth only, in single propositions, then we may run a better chance of hitting it if we make our statements as vague and indefinite as possible. Every finite statement, say Bosanquet and Bradley, can be true only under conditions not explicitly mentioned. To this we might reply that the further statement, "There are conditions under which this first statement is true," must be true absolutely. Pretty clearly, however, if we are sincerely in search of knowledge, it would be better, as a mere consideration of good method, to say: "Rather than rest on truistic generalities, let us take the risk of error. What we seek is not merely truth, but the whole truth; the more complete and precise, the better. By being indefinite, I can avoid error, but at the same time I avoid knowledge." In thus welcoming the idealist call for the "whole truth," I can not see that one is in the least committed to the further metaphysical thesis that the whole world is one unified and necessary system, and any isolated part must be a "false abstraction." In any kind of a world where truth was good, it would be desirable to seek more of it.

I like, indeed, to consider systems quite as well as does Mr. Bosanquet. But here again is a fundamental difference of idealist and realist points of view. Mr. Bosanquet's idea of system is drawn from the Hegelian notion of the "concrete universal." Thus he is led to emphasize the concepts of identity and difference, rather than the concept of relatedness. For instance, the system of colors has an underlying identity. It will be noted that, though they seem

to start as strictly coordinate, the idealist's "identity" always turns out to "underlie" the "difference." The identity in the color system is "color," which is differentiated into reds and blues. But it is not as if one could pick out "color" and leave the red behind, or pick out the red and leave mere "color." Well and good for this illustration. But I do not think that such examples, with their peculiar emphasis on identity and difference, are at all typical of systematic relationships. They lead Mr. Bosanquet, for instance, to think of geometry as differentiations within an identity—namely, space. The result is a bewildering discrepancy between his ideas of geometrical form and those of almost all contemporary geometers. An identity-difference analysis is, I suppose, abstractly possible in almost any sphere. But except where we are dealing with hierarchies of concepts, I see no reason to believe that it is ever the most fundamental or most illuminating analysis. And to fail to differentiate it from a genuinely relational analysis, confounding the two under the common rubric, "system," is to me nothing but blur and confusion. That confusion is the very cornerstone of idealistic logic.

Such insistence upon an "identity in difference" analysis of systems, plus the claim that any part of a whole taken in isolation is a "false abstraction," results, as one consequence, in a denial, not merely of the adequacy of ordinary deductive systems, but of the very possibility of considering "logical form" in any isolation whatsoever. And this in turn has another further consequence, less obvious but of immense metaphysical importance. It makes the world as it now is, necessary, by destroying the legitimacy of even discussing the possibility of its being otherwise. Thus we find Mr. Bosanquet, in his chapter on "Judgment and Supposition," led by the logic of his own position to declare that what we think of as the possible is always nothing but just another revelation of the characteristics of the actual. Mr. Bosanquet tells us, in substance, that while you may make a supposition contrary to fact, the instant you venture to judge, it is about the actual that you judge. Be it ostensibly concerning the supposition only, nevertheless really you infer or judge always fact, not supposition. And of facts there are none, save the aspects of one Fact—Reality as a concrete whole. He, then, who would escape from "the long second-best" of this our actual world into fairer realms of the possible and the ideal, will find there only a shadow cast on nothingness, the mirage of the prison whence he fled. Behold, the actual is the necessary, for there is no other save the actual itself again! Nay, why flee? For all your wants are satisfied by this actual Reality—that is to say, all your *reasonable* wants! Such is the irony of Idealism, that thus transforms

itself, before our eyes, into an almost brutal actualism, and bows down in reverence before whatever has the good fortune to exist.

Again full reply is beyond our present compass. Again I can only make the confession of faith of a realist who still believes in ideals: one who believes that much in this world is contingent, and might well be otherwise; and that it is possible to make true statements about the ideal, when the ideal does not exist, statements that are not queer backhanded ways of characterizing what does exist. I am inclined to believe that there are certain formal principles of structure to which the world conforms, general types of systematic relationship. For example, the so-called "law of contradiction" is, at last analysis, one such principle. But the "law of contradiction" that gets stated in a logic-book is not the principle itself, but its representative, a sort of demiurge who descends into the realm of language, and tells us how to use symbols. Such ultimate principles are perhaps unstatable in words. But if so, it is not because they are mysteriously ineffable, but because they lie deeper than language, deeper than the truth and falsity that words express. Every ordinary statement has its possible contradictory. But the principles to which I here refer have no possible contradictories. What meaning could we give, for instance, to an attempt to contradict the law of contradiction itself. And so also with the other similar principles. Such principles lie deep inwoven into the structure of this world. They are the formal characteristics of systems. They are not premises of deductions or ordinary axioms. They are the ultimate rules according to which the game must be played. If you try to evade the rules altogether, you do not play at all. You could, however, play another game and keep within these same rules. I would not assert that these principles are each "true within its own four corners," independently of all else. I believe that they are actually embodied in all the warp and woof of this world; and, in addition, I furthermore believe that if any other world were real, they would be embodied in it, the very groundwork of its texture. What they would be "in themselves alone," that I know not. They are not apart from this world, but they are not exhausted in it. They are the basis of fact to which we must appeal when we unfold the implications of a supposition. Thus we see that the belief in "logical form," and the belief that the possible may be possessed of a systematic structure of its own, are mutually interdependent.

The idealist declares mind is the greatest thing in the world: for where anything is, there it is. It seems to me unlikely that everything is known, but I can not disprove it, nor does this matter. The serious point of difference is that I would go further than this,

and declare the mind is potentially greater than all that is. I say the glory of mind is in this: that where things are not, it yet can be, and from outside all that is, it can look back upon what is, and if need be, condemn it. I hope I am not here interpreted as declaring that the mind itself becomes unreal. I am still talking in dualist terms. The mind has real thoughts, but these may be about the unreal, about that which only might be. I would go so far as to say that where possibilities are not considered, mind is not. And hence, the necessary and self-enclosed system of an Absolute Idealism of the Bosanquet type, wherein there are no possibles save the actual,—I speak not here of the rather different type illustrated by Josiah Royce,—such an Idealism seems to me very near to being merely a glorified mechanism, a new naturalism, that uses indeed the language of psychology in preference to the language of mechanics, but knows not the meaning of mind. I am far from agreeing with Mr. Bosanquet that mind is the one self-subsistent, self-enclosed thing in the world. I find it rather that thing which is the most relative and dependent. But I give it an independence he does not give it, for I believe that, at every step it takes, mind transcends the world whence it sprang. The Hegelians well say that a thing can only be understood by a mind that contrasts it with what it is not. But then they say, also, that the Absolute, the living synthesis of all that is, need not be thus compared with another which it is not, for within itself it contains what it is not. This last phrase is nonsense pure and simple, and the system ends in self-contradiction.

For the sake of a particular emphasis, in the above criticism I have rather oversimplified. I have perhaps suggested that all systematic relationship is a matter of a few highly formal principles or relationship constants. I should prefer to believe, however, that such "logical systems" are merely the most general and simple sort of systems. Other systems will always illustrate logical systems, but will be something more. And from any system you may get a basis for discussing the possible—not because the system is "real," but because it is a system. I take it that, supposing you placed yourself at Mr. Bosanquet's standpoint, you would consider that you approached nearer and nearer to system as you approached concrete completeness. Anything less than the concrete whole would be, at best, an imperfect system. I should prefer to say rather that all systems are abstract, or better, that they each move on a particular plane. A tree or a man will belong, in general, to more than one system. But a thing will belong to any given system by virtue of one of its aspects or qualities, not by virtue of its concrete totality. While the single individual thing thus runs perpendicular

to systems and ties them together, it does not make them one system. Inference is always within a system. From the fact that a man is a good citizen, you can not infer how fast he will fall if he jumps out the window. In general, no inference can pass merely by way of the individual thing, from one system to another. I am not denying that certain systems may be otherwise related, but I am denying that there is a concrete system of all things.

While I hope I have already made clear the radical divergence between the "identity in difference" analysis that leads to the "concrete universal," and the above sketch of a pluralist theory of systems, a further comment may help to show why I think the latter more fertile in solving the actual problems we meet. The set of scientific laws in a well-differentiated science is a characterization of a natural system. There is no *a priori* reason why there should not exist bodies completely characterized by one or two such sets, for instance, physical bodies. And furthermore, there is no reason why there should not also exist other bodies, such as organisms, behaving as accurately in accordance with the *laws* of physics as do sticks and stones, and yet because they belong also to another superposed system, are not predictable in their total behavior so long as you consider only physical laws. No one believes that mathematical laws are broken because objects also obey physical laws, or the first law of thermodynamics broken because the same things that illustrate it also illustrate the second law. Why then should still "higher" systems be supposed to break or nullify the lower? The only reason for such a supposition is the confusion between the system and its laws and the thing and its total behavior. And this confusion is made equally by the mechanist philosopher and the devotee of the "concrete universal." Each thinks there is only one system; they disagree concerning which it is.

Implication, Mr. Bosanquet tells us in the present volume, is a relation between, or among, the several parts of a system, which so ties them together that a change in one part would call forth a corresponding, though different, change in the others. It is on the basis of such answering of part to part that inference is possible. This definition of implication in terms of "potential differences in a system" (the terminology is not Mr. Bosanquet's) is interesting for the way in which it introduces the notion of an unreal, but only possible, change into the very definition of implication. It is interesting also for the way in which it makes implication a relation in the realm of objects, and not in the more mental realm of truth-values of propositions. I suspect, however, that Mr. Bosanquet would feel rather shocked at my perverse selection of points of interest. Mr. Bosanquet might much prefer to stress the sym-

metrical character of implication: when *A* changes, *B* does; and when *B* changes, *A* does. Now, in spite of the many examples Mr. Bosanquet has scattered through all his logical writings, I see no good reason for believing that implication is thus symmetrical. I am unable to see why *A* and *B* might not be so related that when *A* changed, *B* did, but *A* did not always change when *B* did, and *B* was not analyzable into factors, one of which changed only when *A* did. In such a case we should say *B* implied *A*, but not *vice versa*. And I am unable to see that a system of such implications is any worse system for that fact. A classification of systems into perfect and imperfect on this basis is one I should certainly reject. But here again we face fundamental disagreements, and the full discussion would be a long story.

One aspect of the general problem we should not, however, omit. The unsymmetrical implications which are relations between truth-values of propositions in an ordinary deductive arrangement are certainly subject to peculiar paradoxes. Mr. Bosanquet thinks these puzzles prove that genuine implication is always symmetrical and mutual, and that such notions as those of logical independence and logical priority are mistaken. To me they seem rather to establish that the usual deductive system form is, to no small degree, awkward and artificial. For instance, granting unsymmetrical implication, we should probably say that the implier is dependent, and that which is implied is independent. Again, we might say that that which is implied is logically prior. But then, in one of the usual deductive systems of propositions, the theorems would be independent and logically prior to the initial axioms! Or if the mechanist says the future is rigidly predictable, that would not be because the future is dependent on the present. No, the implier is dependent—he must mean that the present is dependent on the future! You can evade these disconcerting paradoxes by throwing aside such terms as "logically prior," or "independent." But this is a lazy-man's solution. It is much better to grapple with the difficulty. A system of propositions must be considered as a derived and secondary product. Its linear or tree-like arrangement may ill represent the actual simultaneous balancing and interplay of eight, a dozen, a half-a-hundred interrelated elements and factors in an objective system. In emphasizing this inadequacy, Mr. Bosanquet is certainly right. But he would not be willing to follow me in some further details, which I shall try to hint at by an example.

In a system of propositions, deductively arranged, about plane geometry, we start with concepts of point, line, plane, etc. Also we lay down some conveniently assumed axioms and postulates about these. We then proceed to a new synthesis, not really con-

tained in our starting-point, such as, for instance, the concept of a triangle. The direction of logical dependence is clear enough—triangle presupposes straight line, but not *vice versa*. But once given a triangle, there arise a second order of entities, the truths about a triangle. These truths may be independent of the accepted set of axioms, for they might possibly be deducible equally well from some other set of axioms. But once the triangle is introduced—though not before—the truth of the set of axioms, as a set, becomes now dependent on the truths about the triangle. We can then reason: "If not these truths, then not this set; but this set we have accepted, therefore we must accept these truths." The real truth-implication is most adequately stated thus in negative or reversed form, and runs in the opposite direction from the subsequent inference: "If not *q*, then not *p*, but *p*, therefore *q*." So in truth-values the axiom set is indeed logically dependent on the theorems "deduced." There is no real difficulty here; no necessary confusion of sorts of logical dependence. Nothing could be sharper, nothing more precise—though if one must be an idealist, and identify a triangle with the sum of the truths about it, one does get into a mess! The deductive system form is perfectly consistent in itself, once you realize that it is, in comparison with the subject-matter expounded, somewhat artificial.

The deductive system form is artificial. Many a reader of Spinoza has commented on the artificial form of exposition employed in the *Ethics*. But when it comes to mathematical texts, too few readers ever come to appreciate the degree to which mathematical exposition is artificial. Axioms, postulates, deduced theorems, come even to be taken for the subject itself: that *is* mathematics, we say. Then the formalism, and the frequent arbitrariness of order, make a renewed impression on us; and instead of noting that the form of exposition is where the arbitrariness resides, we make metaphysical generalizations about mathematics itself—perchance we decide that mathematics was made by man, an invention, a tool for handling concrete facts, "like a McCormick reaper." And in logic, likewise, the same story is repeated. Even so acute a writer as Mr. Bertrand Russell not infrequently leaves a careful reader in bewilderment whether it is subject-matter or symbols that is intended. Three-fourths of the work of developing symbolic logic has been a partially successful attempt to get the symbols out of the way, so that we could look at the subject. As Mr. Russell himself remarks, a commutative law in an algebra is simply a negative statement to the effect that the order of the symbols is not symbolic, does not symbolize anything. And so it even comes about that logical form itself is supposed, by some good people, to be forms of

words, and we are told that formal reasoning, without considering the subject-matter, is impossible—they not perceiving that the only form of any significance whatever for validity in deduction is the form of the subject-matter itself, its general structural and systematic character. One whole school of the very newest logic is built up round this misapprehension. In short, the usual deductive expository arrangement has repeatedly been openly identified with logic, and tacitly identified with mathematics, yet it is after all only a mode of didactic exposition.

In these matters I should be at one with Mr. Bosanquet, if his criticism of the linear arrangement of propositional implications was simply a movement towards subject-matter and away from arrangements of symbols. But Mr. Bosanquet's reform is also a movement away from specific analysis and particulars, and towards totalities and the Absolute. Even here I can follow a little way. The synoptic general survey is, as knowledge, not to be condemned. It is true, moreover, that the movement of thought actually is from wholes to parts and back again to a new survey of the whole, and not from step to step of well-ordered deduction. Deductions all in a row look well in a book, but they are not the way we think. Deduction is a sort of check on thinking, like "casting out the nines" in arithmetic. Is, then, all thinking inductive? That depends on what you mean by induction. Induction is a term that has become almost useless through being used in two quite contrasted senses. On the one hand, it is applied to the collecting of cases of a particular specified kind. On the other hand it is applied to the whole process by which a realm of knowledge is "whipped into shape." Books on induction soon get over the latter subject: the inductions are not merely collections of cases, they are analyses of cases, and deductions come in to help the process on, including Mill's methods, which are as deductive as syllogisms, and—. But just here some one interrupts, and declares that all that is sound in this induction is deduction; the rest is "lucky guesses." Inductive inference does not exist. But it would be equally fair, or unfair and rather more enlightening, to declare that deduction is nonexistent. All thinking is inductive in this sense, that it is not an inference from next to next on a level of noetic perfection; it is a rising from one noetic plane to another, from the popular to the scientific, from the vague to the explicit. It is the strength of the Bosanquet-Bradley logic that they have made this fact central in their account of knowledge.

Thought seldom proceeds by "linear inference" from the known to the unknown. It proceeds by a jump to a general gross impression, and then it comes down to the more specific details. These details were not in the general impression, but they were suggested by

it. Then there ought to be verification of details by active contact with new observations, and then a remodeling of the general impression—and thereafter the process starts all over again. This movement series has been described as: "preliminary induction, deduction, verification" (Mill), or "hypothesis, deduction, verification" (Jevons and others), or "problem, placing the problem, adducing a new suggestion, deduction, verification" (Dewey). The hypothesis is, however, too often taken to be simply a proposition from which "deductions" are made. But far more fatal is the regular omission of the final step, that of "remolding" the hypothesis in the light of a total synoptic survey of the evidence. There are "the starting place, the operation, and the modification of the starting place." Here Bosanquet and Bradley have made a most essential contribution. They have told us indeed "how we think." The chief limitation of their account of the inductive movement of thought towards a higher knowledge plane is that they leave out the *induction,* in the older sense of the term—that is, the empirical aspect. Looking upon the process as an internal dialectic of coherence within thought, they slur over the empirical checks which actually knock a thought-process into shape by unexpected blows from without itself. Attention to these empirical checks it is the achievement of Professor Dewey's "experimental" emphasis in logic to have kept before us—almost in spite of Dewey himself, for Dewey leans back from "dualism" until he, also, nearly falls over into the internal dialectic account of "experience." With this qualification, Mr. Bosanquet's exposition of inductive thinking, in his larger *Logic,* seems to me, when all is said, the soundest and most illuminating account of thinking as it actually occurs that can anywhere be found in those repositories of tradition, perversion, and untruth, the standard treatises on logic.

As I turn from these all too crabbed criticisms of mine to the delightful limpid flow of Mr. R. F. A. Hoernlé's ingratiating appreciation of Mr. Bosanquet's new volume (in the *Philosophical Review,* January, 1921), and find myself agreeing, after a fashion, with almost everything there said, I ask myself whether it is not wrong so to emphasize the disagreements. But no. Much that we say will sound the same to careless ears. But the exact nuance is everything. The difference in the turn of a phrase really indicates that deep in our hearts there is disagreement, philosophies that are worlds apart. We shall agree that the Atlantic Ocean is salt and two plus two equals four. But when we ask ourselves what we mean even by these statements, the divergence will begin to appear. As for the opinions I have been expounding, I should not wish to characterize the relation of mind and the world as a "dualism."

But the order of nature and the order of knowledge do impress me as observably, empirically different; and I can not grant that the world-evolution is more than in part evolution of thought-content as such. How we think is not generally how things come to be. I agree with Mr. Bosanquet concerning much of his account of the order of knowledge. I agree that the insight into isolated matter of fact is less good as knowledge than the insight, where attainable, into a system of which that fact is but a corollary. I should apply this conclusion to that unfortunate illustration—the insight that "two straight lines can not enclose a space"—which Mr. Bosanquet so often repeats. Mr. Russell was too optimistic when he said that only at the Universities of Llassa and Oxford is this now taken for self-evident truth! I have no insight which I can trust which tells me whether in physical space two straight lines can or can not enclose an area. Whether or no they can is a question whose solution depends on the total evidence for the physical reality of some system chosen from one or the other of two great groups of geometrical systems. But this evidence itself it not merely knowledge of systems. Knowledge of systems can not of itself tell us what is empirically real. Rationalism can open and spread out before us the possibilities; only empiricism can select the true one. I realize that much which to my present knowledge now seems contingent may, on deeper view, be revealed as a necessary element in a wide system; and far be it from me to say that all necessity is in mathematics, all contingency in ethics or art. But still I do believe that deeper yet, in the order of nature itself, in the very heart of things, we come upon contingency again. The world must ever be in some sense brute fact, given datum. Certain systems are real; certain other consistent and possible systems are not. This theory, precisely because it does not identify the real with either the necessary or the ideal, makes, it seems to me, an idealism possible, a love of and aim after the perfect, that is truer to the name idealism than is Mr. Bosanquet's attitude of acceptance and surrender before the Absolute Fact.

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Trinity College, Hartford.

The Control of Ideals. H. B. van Wesep. New York: Alfred A. Knopf. 1920. Pp. 154.

The author attempts to lay his finger on the cause of modern wars, and finds it partly in the multiplication of ideals that accompanies the growth of civilization and their consequent clash, and partly in the deification of these ideals, the belief that they are over-individual and God-made. The solution of the problem he feels

does not lie in reducing the number of ideals (that would be retrogression) but in recognizing that ideals are man-made. Ideals are man-made and should be men's servants, not men's masters. The spirit of sacrifice to ideals has become a mania and threatens to destroy civilization by the very agency that should promote it. Since this mania results from belief in the divinity of ideals, we may be assured that as soon as this belief is dissipated society will be able to harmonize its ideals rationally. By this means wars will be prevented, for the wars of conquest have long been over and modern wars are those due only to the conflict of ideals fanatically worshipped. At bottom, the book is an appeal to society to apply the principles of thoroughgoing individualistic ethics, and the tone of the book is summed up in this sentence, "The individual is the proper unit in ethics because it is the individual alone that is self-conscious" (p. 135).

The book is clearly written in an easy fluent style, entirely free from technicalities and the involved economic issues of present-day ethics. It is eminently suited for a popular audience.

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Pessimism. BENJAMIN F. LACY. Philadelphia: J. B. Lippincott Co. 1920. Pp. 94.

There are various ways of approaching pessimism—metaphysical, psychological, literary. But Mr. Lacy's treatment does not seem to follow definitely any of these ways, nor does his book impress one as having hewn out a new way. There are distinctions made of no great originality—as, for instance, that between the spiritual and empirical pessimist—and there are extended characterizations of the pessimist type, but nothing that is striking or seems to add to the literature of the subject. As desultory conversation by the fireplace such discussions are interesting, but somehow one expects more in a printed book.

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JOURNALS AND NEW BOOKS

REVUE DE METAPHYSIQUE ET DE MORALE. Octobre-Décembre, 1920. *La volonté, la liberté et la certitude d'après Renouvier* (pp. 395–421) (*Suite et fin*): O. HAMELIN. – In a previous article M. Hamelin had shown how volition as conceived in the terms of Renouvier's phenomenalistic psychology is by definition

free. "A motivated action is a free action." Here he gives the evidence Renouvier finds for the actual existence of volition. It is not only moral action which is essentially motivated; an examination of the various grades of belief will show that "a motive is at the root of any judgment." The practical reason is the root of all reason. *La théorie de la relativité et sa signification* (pp. 423–469): E. GUILLAUME. – An interpretation intelligible only to those who can follow the mathematical formulation of recent developments in physics. *Introduction à la sociologie* (pp. 471–494) (*A suivre*): J. WILBOIS. – An outline of a method for social science, suggesting a division of the subject-matter, and leading up to a conception of cause in human society, which will be developed in a later article. *La naissance et la mort* (pp. 495–515): G. SIMÉON. – "Philosophic reflection reveals to us that since time has no existence except through the spirit, the conceptions of a beginning and an end are inapplicable to spiritual activity." *Études Critiques. Quelques livres de philosophie italienne* (pp. 517–536): J. F. RENAULT. – Interesting analyses of the following recent books: P. F. Orestano's *Prologomena to the science of good and evil;* M. F. Sarlo's *Psychology and Philosophy,* and his *Philosophers of our time;* M. G. Saitta's *The thought of Vincent Gioberti;* G. Gentile's *Origins of contemporary philosophy in Italy;* M. Aliotta's *The new realism in England and America,* and his *Eternal warfare and the drama of existence;* M. E. Troilo's *The conflagration, an inquiry into the history of the spirit of to-day.* This recent Italian philosophy is characterized by a platonic idealism in reaction against positivism, and by the traditional juridical spirit of Italian thought. These writers seem to preserve some of Plato's glow and color as well as the familiar metaphysics of platonism. It is claimed that they can make even the American and English platonists of to-day, our neo-realists, seem not only interesting, but entrancing! *Tables des Matières* (pp. 537–541). Index to the 1920 numbers of the *Revue. Supplément.* Reviews of the following: *Œuvres de Maine de Biran* published by Pierre Tisserand; O. Hamelin, *Le système d'Aristote;* Raoul Mourgue, *Étude critique sur l'évolution des idées relatives à la nature des hallucinations vraies;* Albert Kaploun, *Psychologie générale tirée de l'étude du rêve;* Dr. Toulouse, *Comment utiliser la guerre pour faire le monde nouveau;* René Hubert, *Les interprétations de la guerre;* R. Lespieau, *Le molécule chimique;* Floris Delattre, *La pensée de S. H. Newman;* Benjamin Kidd, *La science de puissance;* Benjamin Kidd, *Social Evolution;* H. Wildon Carr, *The philosophy of Benedetto Croce; The problem of art and history;* Benedetto Croce, *Nuovi Saggi di Estetica;* Th. Ziehen, *Lehrbuch der Logik auf positivistischer*

Grundlage mit Berücksichtigung der Geschichte der Logik; L. E. J. Brouwer, *Wiskunde waarheid werkelijkheir. .Nécrologie: Zeuthen.*

Boyer, Charles. L'Idée de vérité dans la philosophie de Saint Augustin. Paris: Gabriel Beauchesne. 1921. Pp. 272. 16 fr.

Boyer, Charles. Christianisme et néo-platonisme dans la formation de Saint Augustin. Paris: Gabriel Beauchesne. 1920. 12 fr.

Brett, George Sidney. A History of Psychology. London: George Allen & Unwin, Ltd. New York: Macmillan Co.
Vol. I. Ancient and Patristic. 1912. Pp. xx + 388.
Vol. II. Mediaeval and Early Modern Period. 1921. Pp. 394.
Vol. III Modern Psychology. 1921. Pp. 322.

Camera, Ugo. Saggio di un sistema di filosofia basato sulla suggestione. Aquila: Officine Grafiche Vecchioni. 1920. Pp. 334. L. 20.

NOTES AND NEWS.

A complete report of the history, methods and results of psychological examining in the United States Army has been recently published in the *Memoirs of the National Academy of Sciences,* Volume 15, 1921. The report is edited by the Lieut. Col. Robert M. Yerkes, Chief of the Division of Psychology, as an official document for the Surgeon General of the Army. It consists of three parts bound in a single volume. Part I, presenting the official history of the development of the service and its activities during the war, is supplemented by reproductions of all of the printed materials devised and used in conducting psychological examinations. Part II includes a complete account of the preparation of methods, their characteristics, and their evaluations as practical procedures. In Part III the results of examining are summarized. The entire report may be obtained from the Superintendent of Documents, Government Printing Office, Washington, D. C., at $1.75 per copy. It appears in quarto size under the title *Psychological Examining in the United States Army,* and includes vi + 890 pages.

Professor A. J. Schneeweiss of the University of Pittsburgh has had his name changed to Snow. The article by him which appears in this issue is accordingly signed with the latter name.